ON THE

ELECTRIC COLUMN,

AND

AERIAL ELECTROSCOPE.

By J. A. DE LUC, Esq. F. R. S.

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1812.

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1. IN my last paper, published in your No. 149, after having shown, that Dr. MAYCOCK had very ably refuted the natural philosophers, who thought that the *galvanic effects* depended on the *electrical energies* of the particles of *matter*; and proved, that it was produced by the action, on each other, of two proper *metals*; I was obliged to dissent from him on two points: 1. that the *electrical excitation* produced by the two metals did not exist during their *contact*, but only at the instant they were *separated*—2. that the *galvanic apparatus* can only be *excited* by a *decomposable fluid*, which is always *decomposed* when the apparatus acts. But I opposed to these two propositions my experiments related in your Journal for June and August, 1810; which demonstrate, that, in the *galvanic pile* the *chemical* effects are independent of the cause of the *electric* effects; the former being only produced when the *electric fluid* pervades a *pile*, in which a *liquid* acts on the *metals* to *corrode them*. I have explained, also, in the same paper, how these ex-

Two points on which the author dissents from Dr. Maycock.

His reasons for this.

* From Nicholson's Philosophical Journal, vol. XXXIII.

experiments led me to the discovery of an *electric apparatus*, which, without any *liquid*, and composed only of alternate *zinc plates*, and equal pieces of *Dutch gilt paper*, produces strong *electric*, but no *chemical* effects.

Spontaneous electric machine of unlimited power and duration.

2. My paper, Sir, in your Journal for October, 1810*, contains the description of a small apparatus of this kind, which, in order to distinguish it from the *galvanic pile*, I have named *electric column*. It is really a *spontaneous electric machine*, the power of which can be increased without limits, by increasing the number of the groups: it is lasting; for I have still the first *column*, (that represented in the *figure*†) which I constructed five years ago, preserving still its power: and indeed there is no reason why it should lose it, as there is no *liquid* to affect the *metals*; for it requires only that the *papers* should possess the small degree of *moisture* of the surrounding air.

Column of 20000 pieces, by Mr. Allen.

3. As to the increase of power of this natural *electric machine* by increasing the *number* of the *groups*, a very ingenious and well-known experimental philosopher, Mr. W. Allen, has carried it to such a degree as to produce very remarkable effects. His apparatus consists of ten *columns*, each containing 1000 groups of *zinc* and *Dutch-gilt paper*, forming together a series of 10,000 *groups*: but they are of a *small* size, so as to be enclosed in glass tubes; a circumstance, the effect of which shall be seen. The following are the phænomena observed by Mr. Allen, which confirm some points which I have stated.

Jar charged by it.

1. Having tried to *charge* a *coated jar*, the *charge* arrived sometimes to such a degree, as to give a *shock* up to the elbows; but at other times it could never arrive at that point. This circumstance is owing to what I had observed of the influence of the *electrical state* of the *air*.

Did not decompose water.

2. Notwithstanding such *electric* power, Mr. Allen did not perceive any production of *gasses* in *glass tubes with water*, made to connect the extremities of these *columns*.

Effect of size and number of plates.

3. That apparatus shows also what I had found with respect to the *size* of the *plates*; that the *size* was indifferent to the *intensity* of the *ultimate effect*; but that the *larger* they were, the *sooner* that *effect* was produced. The *plates* of Mr. Allen being *small*, many minutes were required to *charge* his *jar*.

* Vol. xxvii, p. 81. † Ib. pl. iii.

4. I come now, Sir, to my experiments contained in your Journal for October, 1810, with a *figure*, half the size of the original, of the apparatus to which I shall refer them, but only in their parts concerning Dr. Maycock's system. For these experiments, beside the *electroscopes* at the *ends* of the *column*, there is one in the *middle*. I shall call A, that connected with the *positive* end; B, that at the *negative*; and C, an electroscope placed at the *middle point*: but by taking off the wire 4, it may be made to communicate with any part of the *column*, the *electric* state of which is wanted to be known; and this by means of an insulated *wire*, so soft as to be easily bent, and thus made to connect, by one end, with the electroscope C, and by the other, with the part of the *column* the *electrical* state of which is wanted to be known. Mr. De Luc's apparatus.

5. These experiments, which begin at p. 88 of the same number of your Journal, referring to some TABLES given in the conclusion of my preceding paper*, I think better, for an immediate reference, to copy them here, with this previous explanation, that in all of them, A indicates the *positive* end, and B, the *negative*.

TABLE I. *Insulated Column.*	TABLE II. *B in communication with the ground.*	TABLE III. *A in communication with the ground.*
A	A	A
+ 10	+ 20	0
+ 8	+ 18	— 2
+ 6	+ 16	— 4
+ 4	+ 14	— 6
+ 2	+ 12	— 8
0	+ 10	— 10
— 2	+ 8	— 12
— 4	+ 6	— 14
— 6	+ 4	— 16
— 8	+ 2	— 18
— 10	0	— 20
B	B	B

Different states of the pile.

6. I come now to the experiments. Experiment 7 is thus related in p. 88 of your Journal above quoted. "At the time "when (on account of the *electrical state* of the *air*) there are "simple and equal *divergences* in the electroscopes at the extre- Experiments, showing the motion of the electric fluid in the column.

* Vol. xxvi, p. 265.

"mities

" mities of the *column*, then *positive* at A, and *negative* at B;
" there is no *divergence* in the electroscope C; it is a *neutral*
" point, and is expressed by 0 in TABLE I. If, at the same
" time, any point of the *column*, at a distance from the point
" C, on the *negative* or *positive* side, proportional to one of
" the terms of TABLE I, be tried by the insulated wire, the *di-*
" *vergence* produced is, as exactly as can be expected in such
" experiments, correspondent to that expressed in the *table*,
with its sign either — or +."

7. In *Exp.* 8 are seen the two different cases expressed in TABLES II and III, observed also by means of the *insulated wire*. The case of TABLE II is produced by placing the end B, or *negative*, in communication with the *ground*. In this case, the electrometer at the *middle* point C, which, in the preceding case, was *neutral*, has a *positive* divergence, equal to that before observed at the extremity A, where it is now *double*. There is no *neutral* point perceptible, but close to the end B, whence the *positive* state is increasing towards A, at the rate expressed by TABLE II. If the communication with the *ground* be inversely placed at A, or the *positive* extremity, which is the case of TABLE III; then the only point found *neutral* in the *column* is close to that end A; and thence the *negative* state is increasing up to B, where it is become nearly *double*; and at the middle point C, the divergence, now become *negative*, is equal to what it is at the extremity B, when the *column* is *insulated*.

The effects produced while the metals are in contact.

8. If Dr. Maycock had known these experiments, in which the electroscopes indicate, to the eye, the *motions* of the *electric fluid* in the *column*, he could not have persisted in his opinion, that the *metals* do not produce their *electrical* effects while *in contact*, and only at the instant they are *separated*; for they were not *separated* an instant during the course of these experiments. Besides, I am induced to think, that Dr. Maycock did not even know yet the existence of that new instrument, the *electric column*; or at least had not had the opportunity of seeing any; for each *column*, of whatever *number* of *groups* it is composed, which constantly remain *in contact*, produces *electrical* effects at its extremities, in proportion to the *number* of *groups*.

This equally takes place

9. Dr. Maycock, not having read my papers, might say, that he had applied his system only to the *galvanic pile*; but had he known

known the same paper of which I am speaking, he would have seen, in p. 91, that I have made the very same experiments directly on the *galvanic pile*; and that I found the same gradation of *plus* and of *minus*, from the *middle point*, as in the *column*. Only these operations cannot be so long continued on the *pile*, because its *electrical signs* are diminishing in proportion to the *erosion* of the surface of the *metals*. But Dr. Maycock could not be informed of these *electrical* phænomena of the *galvanic pile*; because, as it appears, he has not used it; and his galvanic observations have been only on the galvanic apparatus of *troughs*, which, as I shall show in a future paper, has deceived him.

in the galvanic pile.

Troughs act differently.

10. I now come to another class of experiments, which, it seems, are also unknown to Dr. Maycock; for, had he read them, he would have found evident proofs of a *irculation* of the *electric fluid*, when the extremities of the *column*, or of the *pile*, are *connected* together. These experiments consist in producing the *connexion* of the *extremities* by different *bodies*, and observing their effects on the *gold leaf* electroscopes. It has been seen, in the above experiments, that, when the extremities of the *column* are *unconnected*, there is an *accumulation* of the *electric fluid* at the extremity A, where the *gold leaves* diverge as *positive*, and a *deficiency* at the extremity B, where they diverge *negatively*. If a good *conductor* be applied to produce the communication between the extremities, the gold leaves *fall* on both sides: if it be a perfect *nonconductor*, their *divergence* is not altered: but if an *imperfect conductor* be applied, they *fall*, in proportion to the *conducting* faculty of the body.

Other proofs of the circulation of the electric fluid in the pile.

11. These experiments begin at p. 91 of the same number of your Journal. I made them for the purpose of ascertaining a very essential point in electricity, that of the best *insulation* of all our *electrical apparatuses*; having found, that the want of a complete *insulation* may lead to errour. *Glass* is the only body used, on account of its solidity, for pillars in all these apparatuses; and it has, in this respect, the essential property not to be *permeable* to the *electric fluid*: but it is not a perfect *nonconductor*; the *electric fluid* moves, though slowly, along its surface, and to prevent it, it is necessary to cover it with some *insulating varnish*. These experiments, therefore, I made first, in order to find out which were the *best conductors*;

Glass requires varnish for a perfect insulation.

next,

next, what was the best *insulating varnish* to cover *glass*; and in their course I ascertained the different *conducting faculty* of various bodies.

Conductors and nonconductors.

12. The general results of these experiments were, that with respect to the *best conductors*, the *glass tubes* with *water* and proper wires, when no *chemical* effect is produced in them, are sensibly as good conductors as *metals*. As to the *insulating* faculty, I found, that *sealing wax*, in which no spirit of wine is added to make it softer, being laid on *glass rods* sufficiently heated to melt it, is equal to the best other *varnish*; for when placed on the extremities of the *column*, with the precautions I have indicated (the want of which produce very remarkable phænomena) these rods do not affect the *divergence* of the *gold leaves*. Lastly, I have given many details of my experiments on intermediate bodies, showing that, in proportion to their *conducting faculty*, each produces a determined degree of *diminution* in the *divergence* of the *gold leaves*.

The experiments prove a circulation of the fluid.

13. The whole of these experiments affords such proofs of a *circulation* of the *electric fluid* in the *column* when its extremities are connected together, and consequently of its *motion*, that, if Dr. Maycock had known them, he could not have had any doubt of these effects. The *circulation* is in consequence of an *accumulation* constantly tending to be produced on the *positive* extremity, at the expense of the other. This tendency continues, though the extremities are connected together; but the *electric fluid* cannot *accumulate* on the *positive*, while a *good conductor* can transmit it instantly to the *negative*; whence it also instantly returns to the *positive*, by the property of the column. But if the intermediate body be an *imperfect conductor*, the *circulation* is lessened, and some *electrical* signs remain at the extremities.

The electrical action not sudden.

14. There is another set of my experiments, which might have made Dr. Maycock doubt of the very ground of his system. He has imagined a certain *property* producing the *electrical* effects; such, I suppose, as that of the *magnet*; which, in consequence, ought to act *suddenly*. If this were the case, when a communication with the *ground* has changed the *divergences* of the *gold leaves*, that communication being removed, the same *divergences* ought to be *suddenly* restored; but it is far otherwise, as may be seen in Exp. 4, 5, 6, of the same paper.

There

There is some sort of impediment to the *motion* of the *electric fluid* along the *column*, probably caused by a *reluctance* in the *zinc plates* to part with the superior quantity of *electric fluid* they must possess when united with *copper*, to produce their *electrical equilibrium*. The consequence is: that, after having observed the *divergence* of the *gold leaves* in both electrometers, if one of the extremities of the *column* be made to communicate with the *ground*; by which the gold leaves *fall* on this side, and they *diverge* more on the other side; it requires a long time, in some cases many hours, for the same *divergences* to be restored. But progressive.

15. There is an entertaining experiment, which may lead to some discovery respecting the physiology of *vegetables*. Each of the *electrometers* of the *column* may be made to imitate the *sensitive plant (mimosa sensitiva:)* for, as the *contact* of one of the extremities of the *column* produces the *fall* of the *gold leaves* on this side, which rise *slowly*; the contact of the *sensitive plant* makes its leaves *fall*, and they also rise *slowly*. This analogy of *slow* effects, pointing out some general analogy between their causes, must render us cautious not to assign hastily to some vague *property* the effects that we may follow distinctly in their process, such as those of the *electric column*; for they may lead us, in time, to the discovery of causes, in those phænomena which now appear the most obscure. Similarity between the action of the column, and that of the sensitive plant.

16. The IId part, Sir, of the same paper in your journal, concerns the *electric column* in its phænomena as an *aerial electroscope*, and contains the observations which I had already made with that instrument. This class of experiments relates to the opinion of Dr. Maycock, forasmuch as they prove, not only a constant *motion* of the *electric fluid* in the *column*, but that some external cause influences much the *rapidity* of its *motions*; an object the explanation of which I had postponed. The rapidity of elective circulation influenced by some external cause.

17. These changes are seen, when the *extremities* of the *column* are not made to communicate immediately with each other; but only by the alternate *strikings* of a body suspended between them, taking some *electric fluid* from the *positive* side, and bringing it back to the *negative*. Now, the more *rapid* is the *motion* of the *electric fluid* in the *column*, the more *numerous* are the *strikings* in a *given time*; and the difference As shown by the aerial electroscope.

is

is very considerable, in different days, and different parts of the same day.

This instrument made known imperfectly,

18. When I first observed that phænomenon, it pointed out to me a new and very interesting object of study: but, according to a plan of observations which I then formed, I was obliged to make many additions to my *column*, which required much time: but the first description which I had given of that apparatus in a paper to the Royal Society, and of its purpose, made it partly known.

and imitated by Mr. Forster.

19. This accidental communication to the public was a lucky circumstance; for before I could have time to do it myself through your Journal, a very ingenious experimental philosopher, Mr. *B. M. Forster*, not knowing it precisely, imitated it in a curious manner: he formed two *columns*, containing together 1500 groups of *zinc* and *silvered* paper, of the small size of my first *column*, and having placed them horizontally, he connected with each extremity a small *bell*, and suspended between them, and very near them, a small brass *ball*, held by a silk thread. When the apparatus was ready, he heard it *chime*, with a sort of buzzing noise on account of the rapidity of the motion of the ball.

Defect of his apparatus.

20. This apparatus had been mentioned in Mr. Tilloch's Phil. Magazine, and having seen there its description, I spoke of it in the same paper of your Journal, p. 103. But since that time, having had the pleasure of making personal acquaintance with Mr. Forster, and corresponding with him, he has communicated to me, from time to time, his observations of this kind of *aeroscope*, which, though in a different manner, indicates also changes in the *electrical state* of the *air:* for, after having chimed for some time, it stops totally, then begins again, and stops; sometimes it chimes for a moment, between long intervals of silence. This is a very curious phænomenon, but there is a want of intermediary *terms* between the *cessation* and *return* of motion. These inequalities are occasioned by the insulation of the little *ball*, it being suspended by a silk thread. Having tried what would be the effect of a greater distance between the bells, I found that it stopped the motion of the little ball, and I soon judged what was the cause of that cessation. When there is more distance, the little ball tending sensibly as much to the *positive* as to the *negative* bell, the difference

ference between these tendencies is not sufficient to surmount its weight, and it remains without motion; but when it is very near each bell, a very small difference of attraction on one side can make it move towards it, whence it is repulsed. The difference, however, between these attractions may be so small, that the little ball remains undetermined, even at that small distance, though the *column* has a sensible action.

21. My plan had been different from the beginning, and thus free from that impediment: it was to obtain a separate *electrometer*, formed of a long brass rod with a large ball at the bottom, and to suspend at the top, by a conducting thread, a small metallic ball. This small apparatus being connected by its upper part with one side of the *column*, the little ball was to diverge; and I intended to have another large ball in communication either with the other side of the *column*, or with the ground, against which the little pendulum should strike, fall, and rise again. This apparatus is represented in the figure annexed to my paper in your Journal for October, 1810. In the same paper, I explained all the difficulties which I encountered, before I could prevent the little ball of the pendulum from sticking to the large ball. At last, however, I succeeded by the means expressed in the figure; and having determined the distance of the second large *ball*, at which the pendulum should never cease to *strike* it by the smallest power of the *column*, the purpose of the apparatus became to *count* the *number* of the *strikings* in a *given time*; which was the precise indication that I had desired to obtain of the smallest changes happening in the power of the *column*.

This did not exist in Mr. de Luc's.

22. This apparatus was ready for observation in the beginning of April, 1810, and in the remaining part of the same paper I related the phænomena, which it exhibited during this month and the following month of May. The tables of these observations are composed of five columns: the first indicates the *days* and *parts of the days* in which the observations were made. The second, the points at which the *barometer* stood. The third, the points of the *thermometer* in the room. The fourth that of my *hygrometer**. The fifth, the *number* of *strikings* of the *pendulum* in determined *times*. 23. By

Observations with this apparatus.

* This instrument has been taken up by a very ingenious Hanoverian gentleman

The phenomena depended on the electric state of the atmosphere only.

23. By comparing the last column with all the others, in the paper above-mentioned, it may be seen, that there is no connexion of the *number* of *strikings* with either the *barometer*, the *thermometer*, or the *hygrometer*, and only with the different *days* and *parts* of the *day*. Which circumstance confirmed me in the idea, that it was only the different *electrical* states of the surrounding *air*, that produced these changes in the power of the *column*; however obscure was still this connexion, for the reasons which I explained.

The inquiry pursued by Mr. T. Forster.

24. This is a new and very interesting subject of experimental and even natural philosophy, and in publishing it in this its infancy, I had the hope that it might lead some attentive observer to follow it up. This hope has been realized, when I have seen in your Journal, that Mr. T. Forster has undertaken that investigation; particularly as I know his talents, being, since that time, personally acquainted with him.

Improvement in the apparatus.

25. I shall only mention farther, that I have made a new step in this pursuit. Knowing by my former experiments, that, though the *size* of the *plates* is indifferent to the final simple *divergence* of the *gold leaf electrometers* at the extremities of the *column*, it is not the same when, one of them *striking* the side, they are reduced to the electrical state of the ground; for they *rise* faster and *strike* again, when the *plates* are *larger*. Applying, therefore, this result of my former experiments to the motion of a *pendulum*, I have constructed a *column*, which, in two connected parts, contains 1300 groups, formed of *zinc* plates $1\frac{1}{2}$ inch square, and equal pieces of *Dutch gilt paper*. This *column* moves a *pendulum* consisting of a *gilt pith ball* the size of a pea, suspended like the other by a *conducting* thread, and placed in the same apparatus, which prevents its sticking when it strikes the large ball. This *pendulum*, guarded against the agitation of the air by a glass case, moves between the two same large *balls*, being near one inch distant from each other, and it has not ceased to strike

gentleman, residing at present at Cumberland Lodge, near Windsor Mr. Hausemann, he has succeeded in every point, and is resolved, from its utility, to construct it for the experimental philosophers who shall desire it.

during

during already two years that it has been constructed: but the *frequency* of its *strikings* is also very various; for I have observed at times forty-five in a minute; but passing at other times by all the intermediate numbers down to hardly one.

26. In this state I must leave this pursuit, on account of my age; but I have learnt, with great pleasure, that Mr. B. M. Forster is employed in constructing also a *column* with *large plates* and a *pendulum*; and that his nephew, Mr. T. Forster takes great notice of the connexions of this phænomena with various circumstances in the appearances of the air, and with diseases. This, in time, may lead to some useful discovery, both for science and for society.

Mr. B. M. Forster constructing a similar apparatus.

27. This new *electrical* phænomenon, so connected with the state of the *air* which surrounds us, cannot but interest many natural philosophers, were it only with respect to meteorology: it is a new *thread* leading in the maze of *atmospheric* phænomena, provided it is not associated with gratuitous hypotheses. This, Sir, has been the object of the IIId part of my paper on the *electric column*, contained in your No. 124, for December, 1810; in which part I have given an abstract of some other *threads* obtained in the *atmospheric* phænomena, considered both in themselves, and in their relation with those exhibited by the spontaneous appearances and disappearances of the *electric fluid*; especially in the great phænomenon of *lightning* and *thunder*. Mr. Hausemann, of whom I have spoken above in a note, having had the opportunity of observing the different *motions* of my *pendulum*, and persuaded that they must have some connection with the *atmospheric* phænomena, has constructed the same apparatus with large plates, and begun regular observations.

Mr. Hausemann also observing with one.

28. I stop here on this interesting subject, having, I think, recalled it sufficiently to show, that Dr. Maycock had not embraced, or considered with attention, all the branches of experimental philosophy connected with the determination of the *nature* and *functions* of a *fluid* influencing almost all the atmospheric phænomena. But in a future paper I shall treat of another part of the same subject, by coming to the idea Dr. Maycock has conceived of the effect of *friction*, to produce

electrical

electrical effects; which will give me an opportunity of examining his system under a different point of view.

I have the honour to be,

Sir,

Your obedient, humble Servant,

J. A. DE LUC.

Windsor, August the 27th, 1812.